# Management methods of two invasive aquatic plants: Effects on mercury methylation, aquatic carbon, and fish abundance

**Joy Cooke Andrews**

## Public Comments

No public comments were received for this proposal.

# Technical Synthesis Panel Review

## Proposal Title

#0338: Management methods of two invasive aquatic plants: Effects on mercury methylation, aquatic carbon, and fish abundance

| Final Panel Rating |
| --- |
| **inadequate** |

## Technical Synthesis Panel (Primary) Review

### TSP Primary Reviewer's Evaluation Summary And Rating:

**This proposal will compare (1) fish abundance, (2) organic carbon in the water column (labile POC and DOC), and (3) contaminant loads in water, sediments, hyacinth, Egeria, and non−endangered fish before and after 3 methods of controlling Egeria beds. Three treatments will be evaluated: chemicals only (currently the main plant control method in the Delta), shredding and mechanical removal (collection of shredded material by a mechanical harvester), and chemicals + shredding and mechanical removal. There will also be an economic analysis of the cost−effectiveness of different treatment methods. Understanding factors that affect mercury dynamics in the Delta is very important to restoration by CALFED and elsewhere in the region (e.g., South Bay salt ponds). Food webs in the delta are also thought to be food limited, and the PIs suggest that methods of controlling exotic plants infestations might alleviate that problem. However, objectives of this project (p. 6) seem vague and not well integrated, and the lack of a mechanistic context (i.e., specific hypotheses about processes) will make the results less generalizable and will restrict their predictive value. In general, details of the experimental design and procedures are lacking, making it difficult to judge their potential success. The questions posed are very general, e.g., “What ecological processes occur**

**in the aquatic nuisance plants … that affect carbon levels, nutrient effects, and mercury speciation", "What are the links between these processes and populations (and mercury levels) in dwindling and diverse native fish populations and their habitats?" and "What effect will physical removal of a large amount of aquatic nuisance plants have on levels of mercury and methylmercury in the San Francisco Bay Delta?" One reviewer notes that there are no experiments proposed that would address the first two questions. Addressing any of these main questions (only three of five that are posed) would require development and investigation of a suite of more specific, testable hypotheses, and would require major projects in themselves. With the questions so broad and the methods often vaguely described, chances of success are very difficult to judge. The project depends strongly on removal of vegetation via three methods by CA Dept. of Boating and Waterways. Letters of support from the latter would be helpful. Details of the treatment and sampling scheme are not provided. The number of samples to be taken from each treated site, and the size of the areas to be treated, are not stated. Moreover, in the experimental design (p. 16), rather than paired treatments at the same site, different treatments will be applied to different sites, thereby confounding effects of location and treatment. The PIs state that their experimental design will be sufficient to detect "orders of magnitude differences among data sets". This seems like unacceptably low power, and may render findings of "no effect" unreliable. The PIs will assume that foodwebs and bioaccumulation of Hg and Se are the same for all fish species as for silversides (p. 22). One reviewer commented that more attention should be paid to the sediments, which are the largest reservoir of inorganic mercury and likely a major pool of methylmercury. In particular, the proposal does not address processes that will shed light on factors controlling Hg methylation, including measurements of sulfate, sulfide, and Fe in the sediments. The approach used for the analysis of the cost−effectiveness of different treatment methods is unclear, as it is not explained how benefits will be quantified. The PIs have reasonably good academic records, and the infrastructure to carry out the project.**

## Additional Comments:

**The method proposed to determine if mercury and methylmercury associated with Egeria is contained within cells or else is part of DOC (p. 18) will not answer that question. The method proposed will only indicate whether mercury in living cells is associated with the cytosol vs. cell–wall components, and will not indicate what might be associated with DOC in the surrounding water that is derived from decaying Egeria. (DOC often contains a large fraction that is derived from cellulose.) Moreover, measuring Chl a is not the same as measuring primary production (p. 18). Several reviewers commented that there is not enough detail in the budget to evaluate whether it is reasonable. It seems excessive for the level of understanding this experimental design will achieve.**

**This proposal will compare (1) fish abundance, (2) organic carbon in the water column (labile POC and DOC), and (3) contaminant loads in water, sediments, hyacinth, Egeria, and non–endangered fish before and after 3 methods of controlling Egeria beds. Three treatments will be evaluated: chemicals only (currently the main plant control method in the Delta), shredding and mechanical removal (collection of shredded material by a mechanical harvester), and chemicals + shredding and mechanical removal. There will also be an economic analysis of the cost–effectiveness of different treatment methods. Understanding factors that affect mercury dynamics in the Delta is very important to restoration by CALFED and elsewhere in the region (e.g., South Bay salt ponds). Food webs in the delta are also thought to be food limited, and the PIs suggest that methods of controlling exotic plants infestations might alleviate that problem. However, objectives of this project (p. 6) seem vague and not well integrated, and the lack of a mechanistic context (i.e., specific hypotheses about processes) will make the results less generalizable and will restrict their predictive value. In general, details of the experimental design and procedures are lacking, making it difficult to judge their potential success. The questions posed are very general, e.g., "What ecological processes occur in the aquatic nuisance plants … that affect carbon levels,**

**nutrient effects, and mercury speciation", "What are the links between these processes and populations (and mercury levels) in dwindling and diverse native fish populations and their habitats?" and "What effect will physical removal of a large amount of aquatic nuisance plants have on levels of mercury and methylmercury in the San Francisco Bay Delta?" One reviewer notes that there are no experiments proposed that would address the first two questions. Addressing any of these main questions (only three of five that are posed) would require development and investigation of a suite of more specific, testable hypotheses, and would require major projects in themselves. With the questions so broad and the methods often vaguely described, chances of success are very difficult to judge. The project depends strongly on removal of vegetation via three methods by CA Dept. of Boating and Waterways. Letters of support from the latter would be helpful. Details of the treatment and sampling scheme are not provided. The number of samples to be taken from each treated site, and the size of the areas to be treated, are not stated. Moreover, in the experimental design (p. 16), rather than paired treatments at the same site, different treatments will be applied to different sites, thereby confounding effects of location and treatment. The PIs state that their experimental design will be sufficient to detect "orders of magnitude differences among data sets". This seems like unacceptably low power, and may render findings of "no effect" unreliable. The PIs will assume that foodwebs and bioaccumulation of Hg and Se are the same for all fish species as for silversides (p. 22). One reviewer commented that more attention should be paid to the sediments, which are the largest reservoir of inorganic mercury and likely a major pool of methylmercury. In particular, the proposal does not address processes that will shed light on factors controlling Hg methylation, including measurements of sulfate, sulfide, and Fe in the sediments. The approach used for the analysis of the cost−effectiveness of different treatment methods is unclear, as it is not explained how benefits will be quantified. The PIs have reasonably good academic records, and the infrastructure to carry out the project.**

# Technical Synthesis Panel (Discussion) Review

## TSP Observations, Findings And Recommendations:

**Management methods of two invasive aquatic plants; effects on mercury methylation, aquatic carbon, and fish abundance**

**The proposal addresses an important question with regard to ongoing management of invasive plants that potentially affect the mercury load in the system. However, the reviewers and the panel agreed that the proposal provided only vague objectives and was poorly integrated. No definitive hypotheses were provided, and important details on experimental design were lacking. In particular, the size of the treatments was not stated, and the effects of location and treatment are confounded by applying different treatments in different areas rather than by pairing treatments within the area. The study would provide low statistical power to detect changes of only several orders of magnitude, and only a correlational understanding of Hg dynamics. (What complexes are present? Do Hg−DOC complexes enhance or decrease methylation?) Specific biochemical processes governing Hg speciation were not adequately addressed, and the appropriateness of using Silversides as a surrogate for other fish species was not evaluated. The cost−benefit analysis would be constrained by the general lack of clear definition of the benefits of the project. Given these wide−ranging concerns, the reviewers and panel ranked this proposal as inadequate.**

**Final Ranking Inadequate**

# Technical Review #1

proposal title: Management methods of two invasive aquatic plants: Effects on mercury methylation, aquatic carbon, and fish abundance

## Review Form

### Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

| | |
|---|---|
| **Comments** | **The goals, objectives and hypothesis are clearly stated and appear to be consistent throughout the proposal. From my perspective the proposed work is timely, relevant and important. I am concerned that the hypothesis as stated in Section VII , last sentence is overly ambitious. That is, if differences in fish populations are seen (and given natural variability etc. there may no be differences), it will be tempting to ascribe the differences to the treatments when there could be many other explanations. The more useful findings will be those that derive from the physical measurements (DOC, Hg, Methyl−Hg etc.)** |
| **Rating** | **very good** |

### Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

| | |
|---|---|
| **Comments** | **The project appears justified relative to existing knowledge and the conceptual model is well articulated. The proposed work is a good combination of research and pilot project.** |
| **Rating** | |

| | |
|---|---|
| | **excellent** |

## Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

| | |
|---|---|
| **Comments** | **The approach is well documented and the investigators previous experience indicates that the work is feasible. The proposed project is particularly appealing because it addresses several aspects of a complex management problem and the results will be useful to managers/decision makers.** |
| **Rating** | **excellent** |

## Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

| | |
|---|---|
| **Comments** | **The approach is well documented and the technical aspects are well established. The probability of success is high relative to the physical parameters and somewhat less for fish populations (see comments above). The scale is consistent with the objectives and the author's previous experience suggest that they are well equipped for this project.** |
| **Rating** | **very good** |

## Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

| | |
|---|---|
| **Comments** | |

| | |
|---|---|
| | Pre-post monitoring and comparisons are a major component of the work and are well thought out. Plans for interpretation are well articulated. |
| **Rating** | excellent |

## Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

| | |
|---|---|
| **Comments** | As mentioned previously the work addresses important management issues involving complex relations among aquatic plants, heavy metals, and fish populations. The results should provide valuable insights into the effects of aquatic vegetation control procedures on water quality. |
| **Rating** | excellent |

## Additional Comments

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|---|
| **Comments** |

## Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

| | |
|---|---|
| **Comments** | The authors have excellent academic records and appear to have the infrastructure to carry out the project. CALFED managers should have good insight into recent past performance based on the results of the author's recently completed CALFED pilot project. |
| **Rating** | excellent |

## Budget

Is the budget reasonable and adequate for the work proposed?

| | |
|---|---|
| **Comments** | **This is a large project and adequate funding will be needed for a successful outcome. However there is not enough detail, particularly in the Subcontract with Mr. Greenfield, to make an informed opinion if the budget is reasonable.** |
| **Rating** | **good** |

## Overall

Provide a brief explanation of your summary rating.

| | |
|---|---|
| **Comments** | **The proposal addresses important management questions regarding the effects of aquatic vegetation removal on water quality with particular reference to DOC and methyl-Hg. The results should provide useful insights for managers and for researchers seeking to understand the complex relations among nutrients, heavy metals, sediments and plants. The rating of "very good" can be interpreted in this case as bordering on "excellent"** |
| **Rating** | **very good** |

# Technical Review #2

proposal title: Management methods of two invasive aquatic plants: Effects on mercury methylation, aquatic carbon, and fish abundance

## Review Form

### Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

| | |
|---|---|
| **Comments** | **A reasonable set of objectives, though broad, are presented. The PI's "hypothesis" that control of major infestations of aquatic nuisance species will impact bioavailable carbon and plant and fish pools of Hg and MeHg if not self−evident, is definitely safe. The research area is deserving of well−focused attention and additional resources.** |
| **Rating** | **excellent** |

### Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

| | |
|---|---|
| **Comments** | **The questions posed are directly related to CALFED goals in several areas (aquatic invasives and Hg−cycling). Aquatic invasives are a major threat to native ecosystems and understanding how potential control strategies impact Hg−cycling is a relevant and timely line of research. The coupling between invasives and Hg is logical, however, the proposal, though well−designed in many aspects, may not be able to, at a process level, provide the data to answer the "whys" behind any observed associations. The justification could have been improved by linking** |

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| | **sites of Hg–methylation in aquatic environments to state variable controls/conditions generated by wetland vegetation/habitats.**<br><br>**I strongly agree that selenium should be examined due to known antagonistic interactions with Hg and MeHg; however, the rationale put forward for the other trace elements is poor. The discussion of the other metals (at all levels) in the proposal is perfunctory at best. Element specific "clean" sampling and processing issues are not addressed.**<br><br>**Speciation (beyond total and methyl mercury) is ill–defined in the proposal. It's a buzz word that should be avoided in favor of specific forms or operational classes of metals. If a model requires a specific conditional stability constant or phase, measure that. It's not at all clear what the PI's are after (beyond total and methyl) and what incremental value the "speciation" will add. The XANES/EXAFS work is novel and interesting, but I question the value in the context of ambient Hg levels and cycling. Similarly, controlled lab studies at much higher Hg levels (to achieve XAS detectability) may address plant translocation and storage of Hg and potentially be useful in examining variations is availability of Hg from solution complexes, but similar and more quantitative (and less expensive) information is obtainable from traditional chemical methods.** |
| **Rating** | **very good** |

## Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

| | |
|---|---|
| **Comments** | **Manipulative experimental approach as described in this proposal, if properly conducted, is likely to** |

**produce more definitive conclusions than if the more typical environmental monitoring design is followed. Excellent in this regard – not simply an observational effort. Excellent paired treatment/control site design (BACI). The scale (meso–pilot) of the manipulations is also a strong point of the proposal – not simply a few small plots of invasives. The choice of four sites for comparison is also excellent, especially since two have been the subject of prior studies. Attempting to "overlap" sites with other on–going investigations is a valuable way to leverage information. Monthly sampling is great, if not overly ambitious.**

**If the invasive species were replaced by a native macrophyte would similar Hg–cycling behaviors be observed? Are wetlands wetlands as far as Hg methylation is concerned? It might be appropriate to include a wetland plot populated with native species as a reference.**

**A large and useful suite of endpoints and target matrices is proposed for inclusion – good, if overly ambitious. However, the sediments are deemphasized, which I believe is problematic. The sediments are the largest reservoir of inorganic mercury and likely a major pool of MeHg. The Brazilian waterweed in many cases is rooted in the sediments. The sediments are a major, if not the dominant, site of sulfate reduction and mercury methylation. The sourcing of Hg available to the invasive plants is not adequately considered or discussed.**

**Though one could certainly attempt to infer mechanisms from a comparison of MeHg levels in fish from treatment and control areas, it would be more insightful if direct measurements of sulfate reduction and Hg–methylation rates were performed in both sediments and associated anoxic bottom waters. In general the proposal suffers from a lack of process information that will shed light on the factors controlling methylation. A total of only 48 MeHg**

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| | **analyses per year spread across 8 sites (4+4) appears to be woefully inadequate to address the impact of the manipulations.**<br><br>**Though sulfate availability is certainly important (so sulfate measurements are valuable), equally, if not more important are sulfide and iron levels, both of which directly affect Hg speciation. Sulfide at higher levels is also toxic to SRB. Measurements of sulfide and Fe are critical. If sulfate levels are depleted then methane should also be measured.**<br><br>**Approaches to representative sampling and compositing for chemical analyses in the various environmental pools should be spelled out better. The proposal sampling discussion focused nearly exclusively on biology. Surface or mid-water column samples may not adequately represent stratification of key variables within the water column. Will the number of larger "priority" fish sampled be sufficient to derive statistically valid conclusions?** |
| **Rating** | **excellent** |

## Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

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|---|---|
| **Comments** | **Would past control measures carried-out at several of the study sites potentially impact this studies result? E.g. residual herbicides in sediments or disruption of vegetation.**<br><br>**Even though the study design controls more variables than most field studies, and in doing so is certainly better able to definitively address the questions posed (e.g. will harvesting of invasives lower MeHg levels in juvenile fish), there will still be many process questions left unanswered – e.g. why did MeHg levels change.** |

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| | **It's not clear that the target fish species sampled from the treatment/control areas, actually reside in those areas for a length of time necessary to acquire the signature of the treatment. They may be sufficiently mobile to smear outcomes. Caged fish cannot replicate that various uptake routes of Hg.**<br><br>**Natural variability in hydrologic inputs/cycles may drive/result in significant inter−annual variability in DOC and Hg−cycling. Periodic flooding cycles is a powerful known promoter of methylation. Though the field plans do attempt to address this through multi−year sampling, it's a limitation that must be considered especially if the treatment plots cannot be considered true replicates on a year−year basis due to previous year's activities.**<br><br>**You cannot effectively quantify primary production by measuring chlorophyll and associated pigments – this can only be assessed using 14C or incorporation O2 consumption.**<br><br>**Shredding of Water Hyacinth does not appear to be a viable and rational long−term strategy for increasing DOC inputs.**<br><br>**The PI's appear to have the necessary permits and approvals to conduct the field manipulations and fish sampling.** |
| **Rating** | **very good** |

## Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

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|---|---|
| **Comments** | **Not Applicable.** |
| **Rating** | **not applicable** |

## Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

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|---|---|
| **Comments** | **Outcomes from the manipulative paired treatment/control design are likely to be more interpretable and applicable than in most environmental studies. A first-rate set of products, directly addressing the key questions are likely to result. The planned data collection suite also appears to fit well into ongoing studies and data structures.** |
| **Rating** | **excellent** |

## Additional Comments

| |
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| **Comments** |

## Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

| | |
|---|---|
| **Comments** | **The PI's are well qualified and experienced and are likely to effectively implement the project, particularly the field (with collaborator assistance) and data dissemination components. I'm particularly impressed by the collaborative nature and outreach potential of the project. A very large analytical effort is also described which is likely to tax personnel and laboratories. I was not convinced by the proposal narrative that an analytical effort of this scope could be completed in a timely and rigorous manner.** |
| **Rating** | **very good** |

## Budget

Is the budget reasonable and adequate for the work proposed?

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| **Comments** | **The ~250K per year budget appears to be reasonable for the effort proposed. Large in-kind "contributions" are shown, which if realistic, implies a very well leveraged project.** |
| **Rating** | **very good** |

## Overall

Provide a brief explanation of your summary rating.

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|---|---|
| **Comments** | **The proposals strengths are it's manipulative experimental approach, an excellent paired treatment/control multi-site design, large-scale field sites, and exceptionally collaborative and leveraged scope. The major drawback, is that while the questions of impact and change are addressed certainly qualitatively and likely quantitatively, the underlying mechanistic reasons may be much harder to tease out.** |
| **Rating** | **very good** |

# Technical Review #3

proposal title: Management methods of two invasive aquatic plants: Effects on mercury methylation, aquatic carbon, and fish abundance

## Review Form

### Goals

Are the goals, objectives and hypotheses clearly stated and internally consistent? Is the idea timely and important?

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| **Comments** | **The goals, objectives, and hypotheses are generally clearly stated. Not all the stated long term objectives are addressed in this proposal, which was a bit confusing. For instance the first objective is described as the determination of ecological processes in the plants of concern that affect carbon levels and mercury speciation (see Executive Summary, but also in the text of the proposal). Yet no experiments are included that would contribute to this area. Likewise, the second objective is stated to be a study of the links between these processes and populations of fish and their mercury levels. But nothing is proposed here to examine the links, which would mean biochemical pathways and processes, just the correlation, which is very empirical and very practical. Vegetation will be managed and changes in fish populations and mercury levels will be documented but nothing is proposed concerning investigation of mechanisms or linkages. The project is large and ambitious, involving quite a few areas of investigation, teams of researchers, and six subcontractors and consultants. The goals are timely and important, especially in view of recent concerns about the levels of mercury from fish in the human diet.** |
| **Rating** | **very good** |

## Justification

Is the study justified relative to existing knowledge? Is a conceptual model clearly stated in the proposal and does it explain the underlying basis for the proposed work? Is the selection of research, pilot or demonstration project, or a full−scale implementation project justified?

| | |
|---|---|
| **Comments** | **The study is completely justified in terms of present knowledge of the area, particularly the interaction of mercury with the biosphere. Smaller scale experiments on aquatic vegetation control/mercury in the Delta have already been conducted by these investigators. They have mobile laboratory equipment, and have established relationships with action agencies e.g. California Department of Boating and Waterways Water Hyacinth Control Program. This work will provide a long term (3 year) investigation of the effects of several different vegetation management schemes on mercury in the environment.** |
| **Rating** | **excellent** |

## Approach

Is the approach well designed and appropriate for meeting the objectives of the project? Is the approach feasible? Are results likely to add to the base of knowledge? Is the project likely to generate novel information, methodology, or approaches? Will the information ultimately be useful to decision makers?

| | |
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| **Comments** | **The general experimental approach seems well designed. Several methods of vegetation control will be applied to four areas infested with water hyacinth and egeria. Subsequent changes in water quality, fish populations, and mercury in the environment will be monitored. Appropriate control areas are included in the study. An economic analysis of the cost effectiveness of the different control measures will be carried out by a consultant. The final products of this research should be knowledge of the efficacy of different vegetation** |

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| | **control schemes in reducing populations of the two exotic invasive plants of concern, knowledge of the efficacy and the relative costs of different control measures in reducing the concentration of mercury in the Delta.** |
| **Rating** | **very good** |

## Feasibility

Is the approach fully documented and technically feasible? What is the likelihood of success? Is the scale of the project consistent with the objectives and within the grasp of authors?

| | |
|---|---|
| **Comments** | **The project is feasible. The general experimental plan has a high probability of success. The principal scientists are experts in the chemical and biological techniques necessary to conduct portions of the project. The scale of this project is unusual– three treatments of significant size at four locations, with corresponding controls with monthly samples over three years. Numerous samples are planned involving water quality, mercury levels in water, plants and fish. In addition, treatments, samplings, and analyses must be coordinated with several subcontractors. The project is beyond the scope of anything the principal scientists have previously attempted. However, it has an excellent chance of being brought to a successful conclusion.** |
| **Rating** | **very good** |

## Monitoring

If applicable, is monitoring appropriately designed (pre−post comparisons; treatment−control comparisons)? Are there plans to interpret monitoring data or otherwise develop information?

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|---|---|
| **Comments** | **The experimental plan will allow pre– and post treatment comparisons. In addition, reference areas (nearby, similar vegetation) will serve as untreated controls. Blank and duplicate samples are planned** |

| | |
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| | during field collections, as checks against contamination. |
| Rating | excellent |

## Products

Are products of value likely from the project? Are contributions to larger data management systems relevant and considered? Are interpretive (or interpretable) outcomes likely from the project?

| | |
|---|---|
| Comments | The products of this work will be a better understanding of the interaction of mercury and methyl mercury with plants and fish in the Delta. See comments on Approach. The principal investigators have in place the means to communicate their results to stakeholders and the general public |
| Rating | very good |

## Additional Comments

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| Comments | The experimental scheme is large and complex; a large number of samples will be taken in a number of different areas over three years and analyses will be conducted on: numerous water quality parameters, different kinds of organic matter defined by particle sizes, in water, as well as mercury and methyl mercury in water, in plant matter, and in fish. The experimental plan also includes an economic analysis of the cost effectiveness of several control methods. The proposed work will gather a formidable amount of data. However, this provides a problem for the reviewer since the number of samples for any particular treatment is not explicitly stated. Thus, four experimental sites are described: Stone Lake Refuge, Tom Payne Slough, Dow Wetlands, and Tracy Fish Facility. However, the size of the area to be treated is not described– is it 10 m2 or a hectare? And the number of samples to be taken from each treated area |

| | |
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| | **is not stated. How many samples will have to be taken at different depths to describe, say, the mercury concentration matrix? This makes it difficult to judge the budget, to put it mildly.** |

## Capabilities

What is the track record of authors in terms of past performance? Is the project team qualified to efficiently and effectively implement the proposed project? Do they have available the infrastructure and other aspects of support necessary to accomplish the project?

| | |
|---|---|
| **Comments** | **The two principal investigators have excellent records of productivity in research area pertinent to the proposed project as well as field experience in the Delta itself. Dr. Andrews has applied new methods of chemical analysis to the problem of mercury contamination in aquatic systems. Dr. Kitting has extensive experience in fish and invertebrate ecology. Both investigators have done previous work in the Delta. Of the contractors, Mr. Greenfield has an MS with a person who has done substantial work in limnology, has written a number of reports for the San Francisco Estuary Institute as well as publications in peer−reviewed journals. It is difficult to judge the record of Mr. Mann . He has written a report for the San Francisco Estuary Institute on economic analysis of aquatic plant management, as well as co−written a report on the same subject for the California State Water Resources Control Board. Two of the contractors− at Stanford Syncrotron Radiation Laboratory and at the Moss Landing Marine Laboratory− are not documented. In the text of the proposal, it is mentioned that there is ongoing work at the SSRL.** |
| **Rating** | **very good** |

## Budget

Is the budget reasonable and adequate for the work proposed?

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|---|---|
| **Comments** | **The budget is difficult to assess. Task 3 requires the** |

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| | **majority of funds (73% of $767,295) and includes all measurements of water quality and environmental parameters. But, as stated above, the number of samples for analysis is not clearly stated. Also, role of the various subcontrators is not clearly defined. I would have preferred to see something like: "Herbicide applications and will be conducted by the Department of Boating and Waterways." "B. Greenfield, San Francisco Estuary Institute, will take water samples after herbicide applications." As it stands, the subcontractors are receiving a large amount of the funds (25%) but with little explicit description of what they will do.** |
| **Rating** | **fair** |

## Overall

Provide a brief explanation of your summary rating.

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|---|---|
| **Comments** | **My overall response to this proposal is ambivalent. The experimental questions are timely, even necessary, for an understanding of the effect of vegetation management on the mercury cycle in fish in the Delta. The track record of the principal investigators is excellent. The general approach is sound and sufficient exploratory work and preparation has been done that the plan should be successful. But the experimental plan lacks crucial details and the roles of the subcontractors are vague and it is not possible to judge whether the budget is realistic.** |
| **Rating** | **good** |